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THE LARGE INTESTINE REGARDED AS A SYPHON.

WITH SUPPLEMENTARY NOTES.

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THE LARGE INTESTINE REGARDED AS A SYPHON.

By R. W. LEFTWICH, M.D., *London.*

THE accepted view, which attributes the movements of the bowel contents to peristalsis, is absolutely convincing as regards the small intestine—with its free mobility, its thick walls, and its relatively small lumen; but many, like myself, must have felt the same explanation inadequate, when applied to the large intestine, and more particularly to the ascending colon. This portion of the large bowel has the smallest muscular development, though it needs the greatest, for its semiliquid contents have to be propelled against the force of gravity for from twelve to sixteen hours out of the twenty-four, and in diseases accompanied by orthopnœa perhaps for weeks or months continuously. Compared with the small intestine, its walls are not only thin, but they have the extra disadvantage of being sacculated. It has feeble circular muscular fibres, with admittedly weaker peristalsis, a large lumen, and very little mobility. Especially is this latter the case when it is unprovided with a mesentery, which, according to Treves, happens in 74 per cent. of individuals. Further, it contains no valves, though even veins have these. Lastly, the cæcum is nearly always found empty. Under all these adverse conditions, it is hard to believe that, in the erect position, its semifluid contents can be propelled by peristalsis, whether assisted or not by voluntary muscular contraction. On the other hand, if it is merely one limb of a syphon, the difficulty vanishes, for its contents will be exactly balanced by those in the opposite limb. Thus, in seeking some other force, syphonage suggested itself. Certainly the large intestine has a remarkable resemblance to a syphon. A syphon consists of a reservoir, a short ascending tube, and a long descending one. So does the large intestine. The reservoir, in this case closed, though compressible, is the cæcum; the short ascending tube, 8 in. long, is the ascending colon; while the long descending tube consists of the descending colon, 8½ in. long, and the rectum about 7½ in.; the total length being therefore about double that of the short tube, and likely to act as a very efficient syphon. The sigmoid flexure has been left out of the account, as its curves probably neutralise each other.

But, it may be said, can a syphon act, notwithstanding that its contents are partly fluid and partly gaseous? To determine this point, I took some glycerin, sp. gr. 1260, and agitated it violently with an equal quantity of air until it was full of minute bubbles. Their presence made no difference to the flow through the syphon. Later, I withdrew the short tube from the glycerin at intervals, and in this way caused to enter the tube three air spaces, each measuring about half an inch vertically. The result was that the current, instead of being stopped, was remarkably accelerated until the air spaces entered the descending tube, when, although it slowed down, the stream continued to flow. The acceleration was obviously due to the relatively diminished weight of the fluid in the ascending tube, and the slowing to the reverse condition. The same result was obtained in another experiment, when a single air space, 3 in. long, was admitted; but, in a third experiment, when the air space measured 5 in., this travelled only as far as the commencement of the descending tube, and the flow came to a stop. The syphon, like the intestine, had a long limb, twice the length of the short one—8 in. and 4 in. respectively; an air space 5 in. long was therefore greater than the difference between them.

It cannot justly be objected that in the cæcum there is no tube dipping to near its floor; but, to satisfy others, in a further experiment, I kept the orifice of the short tube only just below the surface of the glycerin—the flow of course continued as before. Owing to the absorption of some of its water, the contents of the descending are denser than those of the ascending colon, and this greater density, provided it is not sufficient to interfere with the flow, is an advantage rather than the reverse. To demonstrate this, I filled the descending tube with glycerin, and the remainder of the syphon with water, with the result that the flow was greatly accelerated. I must apologise for quoting these trite experiments; I performed them partly for my own satisfaction, partly to convince those whose knowledge of physics had become somewhat rusty. The results, in reality, were a foregone conclusion, for the principle of the syphon would admit of no others. The following experiment is more original:—In a recent post-mortem, having laid open the abdominal cavity, I had the trunk propped up in the sitting position with the tubera resting on two bricks, when a little ooze issued from the anus, but, on gashing the transverse colon, the fæces came rapidly out, and were followed by a gush of fluid intestinal contents. On examining the ascending colon, I found it nearly empty, its contents having fallen back into the now greatly distended cæcum. The admission of air to the bend is a well-known way of stopping the flow of a syphon, adopted by distillers, etc., whose syphons are provided with a tap at that point.

In another experiment, I took two syphons, each closed at the short end by a thin rubber bag; while, at the other end, A had a sigmoid bend, B not. They were inverted and filled. On re-inversion, active syphonage took place, and both bags collapsed; but, while the water in the "colon" of syphon A remained, that in syphon B was sucked back into the bag by the vacuum formed there. After syphonage, therefore, the cæcum forms a Torricellian vacuum, the mouth of the small intestine being closed either by its first kink, or more probably by the extroversion of its mucous membrane, like an incipient intussusception (for which lesion a vacuum would account). The effect of the vacuum upon syphon B shows that the S-trap action of the sigmoid flexure is a necessity; but even with it, if the flexure is straightened out or contain nothing but small scybala, so that air can pass backward, back-syphonage may take place, and concretions be thus sucked into the appendix and excite appendicitis.

A crowning proof would, of course, be the demonstration of syphonage in the dead body; but I foresaw great difficulties there, from the flaccidity of the colon wall and the accumulation, hard to remove, of gas in the transverse colon, and I left this till the last. I have now succeeded in doing this, and particularly in demonstrating the vacuous collapse of the cæcum.

The point in a syphon which it is necessary for the descending current to pass in order to start the flow, when the short tube is full, may be conveniently termed the *balance-point*. The balance-point in the bowel is in the sigmoid flexure. The following is the cycle of events in health. After the partial evacuation of the large bowel, consequent upon defæcation, all syphonage is stopped, the vacuum is broken, and the cæcum and colon slowly fill again. The following morning meal increases both the peristalsis and the intra-intestinal pressure, and there soon comes a time when the fæces are pushed past the balance-point, and syphonage takes place. The rectum and its pouch are filled, and the cæcum emptied. Should the contents of the bowel be very liquid, the quantity may be so large that the syphon endeavours to work after the rectum is filled, and then, the sphincter being unable to resist such pressure for long, the hint to defæcate becomes a command. But, in general, the fæces are more or less clayey, and the syphonage more deliberate. Thus the contents of the cæcum are transferred to the ascending colon, while those of the sigmoid flexure are transferred to the rectum. Voluntary defæcation is accomplished by reducing the cubical capacity of the abdominal cavity. The diaphragm is made to descend by an inspiration, and is fixed in that position by the closure of the glottis. This gives a rigid roof to the cavity. The levator ani in contracting encroaches upon the pelvic cavity. This forms a rigid floor. Then the abdominal muscles contract, and so compress the sides of the abdominal cavity, that the contents of the rectum, aided by the contraction of its muscular walls, follow the path of least resistance, and escape from the anus. The collection in the rectal pouch is that most susceptible to pressure, and escapes first; there is then a pause, during which the pouch is again being filled up from the rectum; another voluntary effort evacuates the second collection; and perhaps a third or a fourth effort may be necessary; and, after all, some fæces may be left in the cylindrical portion of the rectum, which indeed is not very susceptible to pressure from voluntary effort. The expression, "the bowels have been emptied," must not be taken literally. The volume of an ordinary stool is very small compared with that of the contents of the entire large intestine; the bowel, therefore, is practically left full to within a short distance of the "balance-point." Thereafter, the cæcum slowly fills again, till the bottom of the column on the left side is once more pushed past the balance-point, when syphonage recommences, and the other steps follow. In diarrhœa, the time occupied by the refilling may be very short, and the syphon action recur at very frequent intervals. The time is prolonged in health by the concurrent absorption that takes place. Considering the flaccidity of the walls of the colon, the remoteness of the ileo-cæcal valve, the absence of valves in the ascending colon, and the faulty *point d'appui* afforded by the elastic pad formed by the small intestines, it is, to my mind, very improbable that any considerable effect is produced upon the contents of the colon proper by voluntary contractions of the muscles surrounding the abdomen. Indeed, unless the gut is very full, such pressure is just as likely to force the contents the wrong way as the right. Some little effect, however, may be produced upon those of the cæcum and sigmoid flexure, especially if the crouching position advocated by Sir Lauder Brunton is adopted.

Should the call of nature be resisted and postponed, "the contracting rectum drives its contents back into the sigmoid flexure" (Waller), the effort which is made later without a "call" is therefore commonly useless. It may, however, be successful, should the bowel be so full that the strain of attempted defæcation forces the column past the "balance-point," and so sets up syphonage.

A syphon will, of course, not work when horizontal, and, for this reason, it is rare for a person to have to leave his bed at night on account of a desire to relieve the bowels. Under the old view, however, this should be by far the commonest time; for peristalsis is naturally most effectual when the ascending colon is horizontal. Still, by the use of pillows, there is usually sufficient elevation of the arch of the syphon to cause a feeble flow, and, if the contents of the bowel are very liquid, the necessity for evacuation may arise. The same thing will happen if the cæcum is full on retiring to rest.

Syphonage further explains the customary single evacuation per diem. With a continuous flow, and no sphincter above the rectum, one would expect frequent evacuation, and this is the case in such quadrupeds as maintain the horizontal position of the trunk at all times, and have in consequence no syphonage. According to Meade-Smith, in twenty-four hours a horse empties his bowels six or seven times, a sheep four or five times, and a pig once or twice. But the pig, like the dog or the cat, has a habit of resting upon his haunch or haunches, and in that position syphonage may of course take place. Many animals, however, by adopting a crouching attitude in defæcation may unconsciously benefit by syphonage.

The recognition of syphonage is not without its bearing upon treatment. In the treatment of fæcal accumulation by enema, the hips should be raised to prevent syphonage and too rapid regurgitation. The patient should remain quiescent for twenty minutes and then resume the erect position rather suddenly. For the treatment by medicine, a hydragogue purgative is to be preferred. The *bête noire* of the bowels is the sigmoid flexure, which in many individuals is never properly emptied; but syphonage will act upon it in the same way that water flushes an S-trap. In the treatment of diarrhœa it is also well to keep the hips raised so as to retard syphonage.

But, useful as the application of the view here advanced is likely to be to medicine, it is probable that in the remote past syphonage had a far wider range of importance. When man gave up his arboreal habits he lost much of the benefit of his abdominal muscles, and when he adopted the erect position he sacrificed the advantages of a horizontal colon. Had it not been for syphonage, he might have had to revert to his ancestral habits, or, persisting, drop out of the struggle for life.

There can be little doubt that the large intestine, therefore, is rightly "regarded as a syphon," at least when its contents are liquid or semi-liquid. When they are too solid to flow, no syphon will act; but even then there is the advantage that the contents of the long will balance those of the short tube.

I suggest, further, that the force used to move the column of fæces and start the flush is hydraulic. We have a small cylinder, the ileum; a large cylinder, the cæco-colon; and a piston, the column of fæces, inseparable, albeit, from the body to be moved. The difference in the sectional areas will represent a multiplication of the power by six. "There is nothing new under the sun," but it is curious to find that hydraulic lifts, Torricellian vacua, S-traps and syphons, were anticipated, by many thousands of years, in the bodies of our ancestors. The point deserves to be the subject of an additional chapter in Paley. Thus definite functions have now been assigned to the rectal pouch, the sigmoid flexure, the great arch of the colon, the cæcum (two), and the intestinal gas. A feasible explanation, too, has been tendered for the great discrepancy in size between the large and small intestines. The light thus thrown upon a great natural process could not fail to suggest many ideas for improved treatment, but these must stand over for another article.

SUPPLEMENTARY NOTES.

SINCE despatching the proofs of my article in *The Edinburgh Medical Journal*, I have engaged in the preparation of a paper on the same subject for a Society, and some new points require consideration. I find, for instance, that I exaggerated the rapidity of syphonage by taking the *maximum* diameter of the large intestine. The transverse colon sometimes belies its name, and shows a deep V-shaped bend. This should not affect syphon action; but, to settle the matter, I had a tube bent to represent this variation, and obtained the usual results. The demonstration on the dead body was obtained in that of a newly-born child. There are some disadvantages at that age, for the colon is irregular in its course and the cæcum is ill developed. On the other hand, there is no difficulty with the gas; for the intestine is absolutely full of meconium, which, when cold, sets, and forms a soft waxy cast of the bowel. Fixing the body in a sitting position, I divided the ileum, and inserted in it the nozzle of a syringe charged with water. Finding the resistance great, I divided the sigmoid, but without improvement. I then warmed the bowel, and, supporting the colon in something like its normal pioid arrangement, I tried again, and was rewarded by starting the syphonage and seeing the cæcum collapse. I have tried and failed with the adult body, owing to the bowel collapsing, and I have since thought that, in the successful experiment here described, I might have warmed the surface only of the meconium cast, and that what I really dealt with was not fluid, but a flexible waxy cylinder. I must also confess to a lurking fear that some fallacy underlies my other experiment on the dead body.

The observations of experimental physiologists upon the movements of the small intestine have been abundant and satisfactory; but those upon the large intestine are almost negative in character. Nothnagel says, "The movement of the small intestine is incomparably livelier than that of the large intestine"; and until the colon is examined with the trunk erect, the actual mode in which the rigidity of the bowel is obtained for purposes of syphonage may not be certainly known. But providing a hypothesis clears up a number of hitherto inexplicable phenomena, as does that of the syphon, a weak link in the chain of evidence will not justify rejection. To compare great things with small, Harvey himself was unable to explain correctly one very important point; but, luckily for mankind, his conclusions were accepted notwithstanding. The solution of the question which I suggest explains, at the same time, why the large intestine is unnecessarily long, why its walls are sacculated, and why its longitudinal fibres are arranged in bands, are so much stronger than the fibres covering the saccules, and do not extend to the rectum. That being so, there is presumptive proof of its correctness. To descend to a *tu quoque* argument, I might say that, in the practical absence of observations, the peristaltic theory, as all-sufficient in the large intestine, has been accepted upon much more slender grounds; while all the points of structure enumerated above are against it.

A certain amount of rigidity is given to the walls of the large intestine by the dissepiments between the saccules, and I can show that these would be strengthened by muscular contraction. The solution I suggest is, that the longitudinal muscular fibres are in a state of continuous tonic contraction, like those of the sphincter ani. The effect of this contraction will be to shorten the bowel, and at the same time deepen the saccules and thicken the coats. The dissepiments will be strengthened by their increased depth, and by contact between the rising walls of adjacent saccules, while the tonic rigidity of the muscular bands themselves will do the rest. The point is difficult to prove or disprove. Under the influence of shock or anæsthetics the bands will be relaxed, and observations valueless. With animals there is no necessity for syphonage, and none may exist; a negative result, therefore, would mean nothing. But I believe this tonic muscular contraction exists in the lower animals, for I have found the cæcum to be constructed on hydraulic principles in many different types. Even in those in whom syphonage is impossible, the advantage of having a patent and fairly rigid tube, instead of the flabby colon seen post-mortem, must be very great. Without it, the *vis a tergo* of the stream from the small intestine, multiplied hydraulically, would expend much of its energy upon lateral expansion of the colon instead of upon propulsion. But I shall be told that, since the longitudinal fibres of the colon and rectum are continuous, one would expect some signs of longitudinal contraction in the rectum, and this would be easy to demonstrate. It is. For what else can account for the tonic retraction of the anus, seen especially in young adults? It cannot be elasticity, for it disappears under chloroform. There is no vacuum and no muscle in the neighbourhood to account for it. We are therefore reduced to the certainty that it is due to tonic contraction of those fibres which are continuous with the muscular bands. But, while I believe this to be the true explanation, the syphon theory does not stand or fall by it. With a full and valveless ascending colon, in a body which has recently been in the erect position, nothing can account for an empty cæcum but syphonage. If I am asked, Why did not Nature evolve a useful rigid tube? I reply, that such a structure would be highly unfavourable to absorption of fluid. According to Treves, the average measurement of the transverse colon is 20 in.; but no clinical observer, estimating it by percussion, would place it at anything like this. In a young man 5 ft. 9 in. high and measuring 34 in. round the waist, a leather tape 20 in. long was hung, so as to form the letter U, from the tips of the eleventh ribs. It reached to 1 in. *below* the top of the pubic bones. As the site of the transverse colon measured 9 in., what but tonic muscular contraction could keep this horizontal? The mesocolon could, at most, arrange it in waves, which would be detected clinically, if present.[1] Again, without tonic contraction, the life of no arboreal animal, or even of an acrobat, would be worth a week's purchase.

With a fixed fæcal column, the conditions are obviously unfavourable for absorption. According to Boas, the movements seen in the large intestine take the form of "depressions and prominences of the haustra" (*i.e.* saccules) "following each other in regular succession." The fibres covering the saccules are much too weak to have any effect upon propulsion; but this alternate filling and emptying of the saccules would be highly favourable to absorption.

It has been suggested that the longitudinal fibres shorten the bowel over fæcal lumps and then propel them by subsequent elongation. But the power exercised by the *relaxation* of muscle must be very slight, and the suggestion affords no explanation of the arrangement in bands.

I do not propose to discuss in detail the question of the application of the foregoing views to treatment. But I make an exception in favour of appendicitis, upon which, since the illness of a most illustrious patient, something equivalent to a scare has shown itself in many quarters. There can be no doubt that appendicitis is very much commoner than it used to be. Why is this? Is there any new factor in our civilisation to account for it? I believe there are two relatively new causes—massage and the physiological exercises for the obese. Massage is largely used in the treatment of constipation, and in this condition the cæcum is likely to be full. If the object were to force some of the fluid contents into the appendix, nothing would be more likely to accomplish it than deep kneading of a full cæcum. This form of massage should only start at the hepatic flexure; it is very valuable over the transverse colon, as it expels that flatus, the excess of which has been shown to interfere with syphonage. Exercises for the obese, such as touching the toes with the finger tips, are commonly practised whilst dressing in the morning—just when the cæcum is likely to be full. The same tendency to distend the appendix, by pressure upon the cæcum, takes place when straining at stool without a call, or before the motion has entered the rectum. Those who have had appendicitis should never run this risk. Another way in which the appendix is likely to become distended with fæcal fluid is back-syphonage, as already mentioned. Real foreign bodies are very rarely found in the appendix, and I take it that a fæcal concretion, or the nucleus of one, is the dry residue of fæcal fluid, the water of which has been absorbed.

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[1] Normal rigidity plus tumefaction accounts for the solid cylinder felt in acute colitis.

The production of a vacuum in the cæcum suggests a way of emptying the appendix. To effect this, a glycerin suppository should be first used, so as to lessen the obstruction in front. This should be followed by a mild hydragogue purgative, sufficient to produce four or five motions—the patient being made to sit up for a quarter of an hour every two hours. The Torricellian vacuum formed with each syphonage would go far to remove at least the bacilli and their nidus, and might do the same for small concretions, when present. I have treated successfully one case of appendicitis on similar lines to those laid down: but, knowing no better, I used hot applications. Another time I should put a small blister on M'Burney's or Munro's point, since, as Treves insists, a certain amount of inflammation may be beneficial by forming limiting adhesions. The blister would probably be quite as efficient as diffused heat for reducing the tumefaction of the appendix.